

Stockholm

December 9, 1957

Professor George Klein  
Department of Tumor Biology  
Karolinska Institutet  
Stockholm 60, Sweden

Dear George:

To yours of the 28th:

I am sorry to hear that Hayes will not be on the program; perhaps he might change his mind at the last minute. However, the other group will make a very acceptable discussion panel.

Frankly I am a little bit peeved about having put Szybalski on the list under the present circumstances. I foresee nothing but boredom at the very best. Frankly he has been a rather disruptive and unproductive influence in genetics in this country. If you want to see what I am driving at, you might look at the article by the Japanese workers in the June, 1957, issue of the Journal of Bacteriology. I am a little afraid that he might use the occasion of the symposium for some similar nonsense, which under the circumstances might waste as many more man months as have been spent cleaning up his sloppy work in this particular subject just referred to. He is, however, a very affable fellow and one hates to do anything that would offer him the least personal offense.

I think it would be delightful to have a set of discussants who would actually discuss the papers being presented. This is another reason that Szybalski doesn't quite fit. It would be quite appropriate if the discussants could have at least a general abstract of what the principal speakers would be talking about so they can frame advance questions. However I hope it would be most clearly understood that the discussion program shall not be used as an opportunity for the presentation of further independent papers. This is a matter that can very easily run away with itself and I think some firmness may be necessary in order to keep the panel on a definite course. So I would accept whole-heartedly the "general standpoint from the organizing committee" with the exception of that sentence that indicates "that the panel participants can give short accounts of such parts of their own work that is pertinent to the discussion". You know very well yourself what will happen if that is encouraged.

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There is no point in dwelling further on the Szybalski question since the milk seems to have been already spilled. However, the framework of the symposium can perhaps be set up in such a manner as to make the best of it. Since it is to last certainly not longer than three hours and thirty minutes, there would hardly be time for one full set of papers by the principal speakers, a set of subsidiary papers by the secondary speakers and discussion as well.

I am still waiting to hear from my collaborator, Gus Kossal, about the final results of our experiments on antibody formation. The preliminary experiments were most exceptional and there is no question at all about the applicability of the method. I hope that you will be hearing from me further about the possibility of some more experiments involving the test of tumor lines for their capacity to produce antibodies.

With all best regards,

Yours sincerely,

Joshua Lederberg  
Professor of Medical Genetics

JL/ew